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Mr. Baily presented a paper "On the right ascension of γ *Cassiopeæ*." As this paper is a short one, and of an interesting nature, we shall give it nearly in the words of the author:

"On comparing the Catalogue of Stars, recently published by this Society, with the catalogue of 100 principal fixed stars given by Mr. Pond, at the end of the *Nautical Almanac* for 1829, I was struck (he says) with the considerable difference which appears in the $\mathcal{R}$ of γ *Cassiopeæ*: Mr. Pond making the $\mathcal{R}$ of that star upwards of one second (in time) more than the catalogue printed by this Society. At first I imagined that some error might have crept into the calculations of the Society's Catalogue, notwithstanding they were made by two computers, independent of each other, and afterwards revised by our indefatigable Secretary, Mr. Stratford. I therefore, for my own satisfaction, went through the whole computations myself, and was pleased to find that there was not the slightest difference in the results. I next reduced the whole of the observations of that star made by Mr. Pond at the Royal Observatory at Greenwich, and found them to agree very nearly with the result deduced by Mr. Taylor, who makes the mean of 10 observations to be $= 0^h\ 46^m\ 13^s,23$ reduced to Jan. 1, 1825: whereas the Society's Catalogue gives only $0^h\ 46^m\ 12^s,13$ on January 1, 1825; being a difference, as already stated, of $1^s,1$. Bradley has 5 observations of this star, and Piazzi has 46. It is scarcely possible that Bradley should be in error one second of time; and still less probable that the mean of 46 observations by Piazzi should be erroneous to such an amount. The result of Bradley's observations (after allowing for the effect of precession) differs $8'',5$ in space from those made by Piazzi; which quantity, divided by 45 (or the interval of years between the two observers), will make the annual proper motion of the star, if it really have any, about $0'',19$ in space. But this is not confirmed by the recent observations of Mr. Pond; and we must look elsewhere for a solution of the difficulty. That the position of a star of the third magnitude should be so undecided at the present day that its $\mathcal{R}$ cannot be satisfactorily depended on, to a second of time, does not speak much in favour of modern astronomy; and shews us that a great deal still remains to be done towards establishing the *fundamental* parts of the science. * * * Amongst other suggestions I have imagined that it might arise from a typographical error; and, in fact, if we suppose a misprint of $10''$ in the $\mathcal{R}$ of γ *Cassiopeæ* in Piazzi's Catalogue, that is, if we read $\mathcal{R} = 11°\ 11'\ 17'',6$ instead of $11°\ 11'\ 7'',6$, the whole difficulty will vanish, and the results of the observations of Bradley, Piazzi, and Pond, will agree to the greatest exactness.

But we are scarcely warranted in making such an alteration without a reference to the original observations. Some suspicion, however, is excited that the printing is not strictly correct, from the circumstance that Piazzi considers the annual proper motion in $\mathcal{R}$, as deduced from Bradley's observations, to be $= 0$; which would agree with the amended reading as here suggested; but which does not accord at all with the present reading in the Catalogue, since the annual proper motion is, as I have already observed, in such case $= -0'',19$.

"Before I close these remarks, I would observe that there are also differences in the $\mathcal{R}$ of three other principal stars (besides that of β *Scorpii*, which is acknowledged to be an error in the Greenwich catalogue) to which I am desirous of calling the attention of the Society; and for which I can by no means account. These are ζ *Ursæ Majoris* and β *Cephei* (both stars of the third magnitude) and *c Draconis*, a star of the fifth magnitude. The two last differ, as in the case of γ *Cassiopeæ*, above a second in time from the catalogue of Mr. Pond: but ζ *Ursæ Majoris* differs as much as $1^s,4$. The case of this last star is the more remarkable, since the observations of Bradley and Piazzi correspond with wonderful exactness; there being a difference of only one second, in *space*, between them, after a lapse of 45 years: whereas from the time of Piazzi to the year 1825, a period of only 25 years, there appears, from Mr. Pond's observations, to be a difference of upwards of 20″.

"The whole of the computations relative to the positions of these stars, I have frequently repeated, and can assure the Society that there is no error in the results, as printed in their catalogue. Time only, and further observations, can clear up these apparent difficulties."

A paper was read "On Double Object-glasses, by M. Littrow."

The first part of this communication is devoted to the derivation of the equations expressing the conditions proposed by Mr. Herschel for the destruction of the aberration of sphericity in a thin double object-glass, by the ordinary processes in use among the German geometricians for such investigations. The author states himself to have entered on this investigation with a view to disseminate a knowledge of the theory alluded to in his own country; but being induced thereby to resume some former investigations of his own, he takes the opportunity to communicate to the Society his own principal results.

Taking for granted the well-known expression of Euler for the aberration of a lens of two surfaces, and developing it in descending powers of the distance of the radiant point from the lens, he obtains expressions, from which, by proper management, and substitution of similar quantities for a second lens, he derives the two final equations (A) and (z) demonstrated by Mr. Herschel. He then proceeds to the main object of his paper. This may be briefly stated to be the embodying of the relations expressive of the refraction of a ray through any four spherical surfaces, however situated,

(provided they have a common axis), in trigonometrical equations, in which no quantity is regarded as small, and of course nothing neglected. These equations are in themselves sufficiently simple when undeveloped, and in that state may very readily be applied to determine whether any proposed construction of an object-glass really does satisfy the essential geometrical conditions of a perfect telescope, by producing a rigorous union of different coloured rays, and rays incident on different parts of the object-glass. Accordingly the author instances their application to a construction recently proposed by a German optician, as of peculiar excellence. In this construction the indices of refraction of the crown and flint lenses being respectively 1·53 and 1·60, and their dispersive ratio 0·25, the thickness of the crown lens 0·1, that of the flint 0, and the lenses being supposed in contact, the radii of the surfaces are,

For the crown	1st surface (convex)	0·69281
	2nd surface (convex)	2·255319
For the flint	1st surface (concave)	1·543030
	2nd surface (concave)	5·768005

Substituting these data in his equations, he finds that they satisfy sufficiently well the conditions of achromaticity, but that they are very far from being entitled to the same encomium when the spherical aberration is considered.

But when the question is inverted, and the problem is, not to try whether a proposed construction be good or not, but, *à priori*, to determine what is best, the equations in question, though simple enough in their trigonometrical form, become complicated by the algebraic developements their direct analytical resolution necessitates. Now the essence of M. Littrow's proposed method is to do away with all this developement, so far as it tends to produce complication; and after preparing the equations in the most convenient manner the case will allow, to substitute for their direct algebraical, an *indirect numerical* solution, by the well-known and ready method of trial and error, a method perfectly correct in theory as well as easy in practice.

M. Littrow exemplifies this theory by applying it to that particular case when the indeterminate problem is limited by the condition of Klügel, viz. that the refractions at the first two surfaces shall be as small as possible, in which case the first or crown lens is in fact wholly given, and it only remains to determine the radii of the flint. He takes into consideration the thickness of the former, but not the latter lens, and neglects also their distance *inter se*. As a numerical instance, he supposes the indices of refraction 1·53 and 1·58, the dispersive ratio $\frac{2}{3}$, and the thickness of the crown lens one hundredth part of its focal length, when, by applying the process described, he finds the following radii:

Crown lens..	1st surface (convex)	0·186823
	2nd surface (convex)	0·608170
Flint lens...	1st surface (concave)	0·407996
	2nd surface (concave)	0·445808

Focal length of the compound lens = 1; aperture 0·06495

M. Littrow, however, is by no means of opinion that the condition of minimum refraction here assumed is the best, or in any way essential to a good object-glass; only it facilitates calculation. He regards it as more advisable to aim at increase of aperture, and for this purpose to assume the first or crown lens equiconvex, which he affirms to be the condition requisite for the attainment of that end. He therefore explains his method of proceeding in this case, and applies it to the same numerical data with those last mentioned, which give for the final results:

Crown lens (equiconvex) Radius of each surface .. 1·06
Flint lens (biconcave) Radius of 1st surface ... 1·04394
of 2nd surface ... 3·296512

Focal length.. 3·702231
Aperture..... 0·09973 × focal length

or nearly $\frac{1}{10}$ of the focal length, being a much larger proportional aperture than it has been usual hitherto to give to achromatic telescopes.

The author concludes by stating the reason of his entering into these investigations, viz. an application made to him by an artist of Vienna.

A paper was also read from M. Slawinsky, containing the following observations made at Wilna. (Longitude $1^h\ 41^m\ 12^s$ East of Greenwich.)

1. *Eclipses of Jupiter's Satellites.*

			Sidereal time.	
1825. Jan. 17	Im. 1 ..	$4^h\ 0^m 23^s,7$	good observation.
Mar. 22	Em. 2 ..	8 51 51,9	very good.
Apr. 23	Em. 1 ..	10 44 25	good enough.
May 5	Em. 1 ..	12 29 38,7	very good.
1826. May 8	Em. 1 ..	15 31 56,1	middling.
June 2	Em. 2 ..	16 46 0,1	middling.
1827. Mar. 18	Im. 1 ..	16 1 14,9	a little doubtful.
Apr. 18	Im. 1 ..	16 46 15,2	passable.

2. *Occultations.*

			Sidereal time.	
1825. Apr. 1	e Leonis ...	Im.	$10^h\ 8^m 55^s,2$	very precise.
		Em.	11 15 14,2	a little doubtful.
23	Star (mag. 8·9)	Im.	11 22 29	exact.
1826. Feb. 15	208 Tauri	Im.	6 55 58,8	good.
16	Saturn			
	1st ansa	Im.	3 11 58,8	good.
	1st limb		3 12 8,8	
	2nd ansa		3 14 48,3	
1827. Mar. 18	λ Libræ.....	Im.	17 4 55,9	good enough.

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